

The procedures had limited success in retaining subjects for treatment. Four subjects completed treatment and the 1-month follow-up, six did not complete treatment, and one completed all three treatment sessions but did not return at follow-up. Of the 33 total sessions (three sessions and 11 subjects), in 12% the intensity of the cravings increased, in 40% the cravings decreased, and in 48% no change was seen. Perceived control over the cravings was generally strengthened after each session. Of the four patients who completed treatment, cocaine use decreased for two, remained the same for one, and increased for one.

Overall, the direct effects of eye movement desensitization and reprocessing on the intensity of cocaine cravings and use appear negligible in contrast to the bulk of the research on behavioral treatments for cocaine dependence in this setting, where significant reductions in cocaine use and cravings are often seen (6). We noted that the subjects with higher levels of cocaine cravings and use may be poorly suited for this approach because the three patients with the highest levels of cocaine use and cravings dropped out of treatment. Our findings suggest that eye movement desensitization and reprocessing have limited effects on cocaine cravings or use in this setting.

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#### Gabapentin Treatment for Insomnia Associated With Alcohol Dependence

TO THE EDITOR: Gabapentin is approved as an anticonvulsant and has also been used to treat chronic pain, restless legs syndrome, bipolar disorder, anxiety disorders (1), and alcohol withdrawal (2). Gabapentin has no known abuse potential, has few side effects, does not require blood monitoring, and does not affect liver metabolism or the excretion of other medications (3). The mechanism of action for gabapentin is unknown. It may influence the synthesis of  $\gamma$ -aminobutyric acid and glutamate (4). These systems are known to modulate anxiety, arousal, and sleep. Gabapentin may also increase deep sleep (stages 3 and 4) by increasing serotonin levels (4). Alcohol-dependent patients commonly complain of insomnia during early recovery, and sleep disturbance has been associated with relapse in alcoholics who are recently abstinent (5).

We used gabapentin to treat 15 of 17 consecutively evaluated alcoholic outpatients who had persistent insomnia de-

spite 4 weeks of abstinence. All patients signed a consent statement (per the Michigan mental health code) before receiving psychotropic medication. One patient discontinued gabapentin treatment after one dose, and another patient refused treatment. The remaining patients included 12 men and three women (aged 24-45 years). Seven patients also abused marijuana, cocaine, or opiates; six had stabilized bipolar disorder, major depression, schizophrenia, or sleep apnea. Their dose of gabapentin was titrated to overall sleep response (mean=953 mg/day, range=200-1500) within 2 weeks. Most patients started to improve with 600 mg at bedtime. While seven patients required a dose of 900-1200 mg/day at bedtime to optimize their sleep quality, one responded to 200 mg/day at bedtime. Three patients took 1500 mg/day in divided doses to maximize sleep quality and reduce daytime anxiety. Neither serious side effects nor tolerance was reported.

All patients remained totally abstinent after 4-6 weeks of follow-up, except two patients who had four or more drinks on one occasion each. Each patient showed improvement on the Sleep Patterns Questionnaire (6), a self-administered measure of insomnia, over the past month; their scores ranged from 0 to 20. Scores on the Sleep Patterns Questionnaire decreased from a mean of 15.9 (SD=2.8) before treatment with gabapentin to 5.9 (SD=3.3) after 4-6 weeks of gabapentin treatment (paired *t* test:  $t=-11.05$ ,  $df=14$ ,  $p<0.001$ ).

Although our study was limited by small group size, the lack of a control group, short-term outcomes, and the lack of polysomnography, we conclude that gabapentin shows promise as a safe and effective treatment for alcohol-dependent patients with comorbid insomnia during early recovery. Controlled studies are warranted to confirm these preliminary observations.

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#### Earnings Changes

TO THE EDITOR: The article by Mingliang Zhang, Ph.D., Kathryn M. Rost, Ph.D., and John C. Fortney, Ph.D. (1), provides important insight into the real cost of the treatment of depression for patients, rather than the cost assumed by third-party payers. The authors suggest that the reason for the cost savings for patients treated by psychiatric specialists

over those treated in the general medical sector stems from specialists' "more extensive training and expertise in treating mental health problems" (1, p. 112). Twice the number of patients treated by specialists as patients treated in the general medical sector had treatment in accordance with Agency for Health Care Policy and Research guidelines (2, 3). However, only 48% of the patients treated by specialists had treatment that followed those guidelines. Because the authors did not provide the details of their analysis, we cannot reach a conclusion regarding the meaning of this finding. Which of the guidelines were not followed? What was the use of certain medications, the dosing, the length of treatment, and the like? If 52% of the specialists were not in compliance with the guidelines because they use antidepressants other than those recommended by the Agency for Health Care Policy and Research, the meaning of the authors' finding would be very different from what it would be if the specialists' noncompliance were based on their failure to make patients' drug levels reach accepted therapeutic doses. This is equally true for the 79% of the physicians in the general medical sector who were not in compliance with the guidelines. When training psychiatrists and nonpsychiatrists about the treatment of depression, knowing what they do and do not do would guide the educational endeavor.

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## Dr. Rost and Colleagues Reply

TO THE EDITOR: In our article, we reported greater cost savings for patients treated by psychiatric specialists than for those treated in the general medical sector. One explanation for this is that 48% of the community residents treated for depression in the specialty care sector received guideline-concordant care in contrast to 21% of the community residents treated for depression in the primary care setting. In his letter, Dr. Muskin requested further information to identify where the deviations from guideline-concordant care occurred in both sectors. We are happy to provide a further breakdown without extensive statistical comparisons. Specialty care patients were more likely than primary care patients to get any antidepressant medication (64.3% and 53.0%, respectively). Among those who received antidepressant medication, specialty care patients were more likely than primary care patients to be prescribed a guideline-concordant dose (52.8% and 39.4%) and were somewhat more likely to take the medication for a minimum of 8 weeks (75.0% and 70.5%). The remaining difference in guideline-concordant treatment rates was explained by the greater likelihood for specialty care patients to report that they received

eight or more counseling visits for depression. The 1992 medication patterns do not adequately represent current primary care medication prescribing patterns, particularly the greater use of newer-generation antidepressants whose limited side effects allow physicians to prescribe therapeutic doses more readily. However, if specialty care's achievement of better outcomes is in part attributable to providing psychotherapy (in combination with medication or independently), we might continue to observe these outcome differences if the study were to be replicated in the current health care environment.

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## Clinical Trials and Effectiveness Research

TO THE EDITOR: Kenneth B. Wells, M.D., M.P.H., has made an important contribution with his article (1). It may be helpful to keep the following considerations in mind in interpreting both efficacy and effectiveness studies for both clinical and policy decision making.

Outcomes are most often defined in terms of easy-to-measure markers. Features such as autonomy or authenticity are rarely considered and are also never measured in either type of study. Yet they are most relevant in terms of the utility of given outcomes to individuals, as well as our culture and our society (2–4). More easily measurable outcomes continue to dominate the studies reported even in this *Journal*. Should all outcome studies carry at least an explanation of why the particular outcomes were chosen and why meaningful outcomes were not considered?

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## Dr. Wells Replies

TO THE EDITOR: Dr. Bursztajn raises the question of whether the authors of treatment-outcome studies should justify their outcome selection within a broader scheme that includes effects relevant to practice and policy that are difficult to measure. The simple answer is "yes," but this is not a simple question.

Practice and policy decisions involve tradeoffs among treatments and their expected outcomes under conditions of constrained resources, uncertainty, and personal distress. The information required to inform those decisions ideally includes all expected benefits and costs, including respect for individual autonomy and societal implications, of the alternative actions. In addition, one would desire a validated pro-